ISOSTASY, A REJOINDER TO THE ARTICLE BY HARMON LEWIS

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In the *Journal of Geology*, Vol. XIX, No. 7, October-November, 1911, pp. 603–26, there was published an article by Mr. Harmon Lewis, entitled "The Theory of Isostasy," which is a direct attack on three publications by John F. Hayford, namely, *The Figure of the Earth and Isostasy from Measurements in the United States*, *Supplementary Investigation in 1909 of the Figure of the Earth and Isostasy*, both published by the Coast and Geodetic Survey, and "The Relations of Isostasy to Geodesy, Geophysics and Geology" published in *Science*, pp. 199–208, February 10, 1911. The attack is direct and positive. Hence this rejoinder is written in a similar manner. The critic may not reasonably object to having his article treated in the manner in which he has treated the publications criticized.

Mr. Lewis claims in his article that Hayford has made a fundamental error in his geodetic investigation, an error in method which vitiates all the conclusions reached. The greater portion of the article is devoted to setting forth this alleged error of method and its consequences. A few pages in the article are devoted to the proposition that since the theory of isostasy does not explain all of the geological facts which have been observed, isostasy probably does not exist.

Hayford believes that Mr. Lewis has in his article, probably unintentionally, overstated even his own extreme views. Hayford is certain that the alleged fundamental error in method in the geodetic investigation, which Lewis sets forth, is not an error and, moreover, that if Lewis had followed his own line of thought to its logical conclusion, he would have convinced himself that no error had been made. The forms of statement used by Mr. Lewis lead one to think that he has some positive basis for other

conclusions from the geodetic evidence than those given by Hayford. A careful reading of the article shows, however, that Lewis has simply suggested that other conclusions are possible. Hayford believes that the arguments in Mr. Lewis' article which are based on geological evidence are essentially weak and in part erroneous.

This rejoinder is an attempt to show briefly some of the basis for the writer's beliefs as expressed in the preceding paragraph. In the first part of this rejoinder practically all of the evidence cited was available to Mr. Lewis before he wrote his paper. In the latter part some new evidence, from gravity observations, is utilized which has not heretofore been accessible to Mr. Lewis but which is now accessible to all interested persons.

Mr. Lewis claims that Hayford made a fundamental error in that "the most probable depth was calculated on the assumption of completeness. If the assumption of completeness was wrong, the depth of compensation which would appear most probable would not be the true depth of compensation but a depth which would counteract the effect of the wrong assumption in regard to the completeness" (p. 612). Referring to the same matter, on p. 625, Mr. Lewis writes, "It is believed that Hayford made an error in determining the degree of completeness of compensation which invalidates his conclusions, for he assumed complete compensation in calculating the depth and then used this depth to calculate the degree of completeness." In contrast to the published conclusions by Hayford,[1] Mr. Lewis writes in connection with the same matter (p. 612), "We are forced to conclude that, from the geodetic evidence alone, neither the depth nor the degree of completeness of isostatic compensation can as yet be considered settled."

Recur for a moment to Mr. Lewis' statement (p. 612), that "If the assumption of completeness was wrong, the depth of compensation which would appear most probable would not be the true depth of compensation but a depth which would counteract the effect of the wrong assumption in regard to completeness." This statement involves in itself an assumption which is absolutely essential to Mr. Lewis' whole argument, namely, that the counter-

[1] See *The Figure of the Earth and Isostasy*, pp. 164–66, 175, and *Supplementary Investigation*, pp. 59, 77.

action suggested by him is possible. Now it happens that such counteraction is nearly impossible to any appreciable extent in the computations actually made. If Mr. Lewis had followed his own course of reasoning farther than he did, using the data printed in the publications criticized he would have convinced himself of this.

He carries his reasoning farther on pp. 616–17 than elsewhere in his article. In the paragraph commencing near the top of p. 616 he shows that if with assumed complete compensation the assumed depth of compensation is made smaller, the reduction factors for each ring become smaller and that, on the other hand, if the isostatic compensation is assumed to be less than complete, the reduction factor for each ring becomes larger. It is also shown that when both these changes in assumption are made at the same time, the reduction factor becomes smaller for certain rings and larger for others. On p. 617 a concrete case of this kind is shown. The table there printed shows clearly that if in the place of assumed complete compensation extending to depth 113.7 km. one assumes a compensation but one-half complete and extending only to the small depth 19.29 km., the reduction factors for rings 29 to 13, corresponding to topography within 56 km. of the station, are all made smaller and those for the remaining more distant rings are all made larger. Thus far Mr. Lewis' reasoning and results are all correct. For convenience, call such reduction factors as those shown in the middle column of the table on p. 617, Lewis factors, and call those such as are shown in the last column, Hayford factors.

At this point Mr. Lewis' logical process begins to go wrong. For at this point the tacit assumption is made, though not stated, that if the Lewis factors are smaller for some rings and larger for others than the Hayford factors the computed topographic deflections with the isostatic compensation considered will be about the same in the two cases. If so (that is, if the computed deflections are easily made about the same), then with effort corresponding to that already made by Hayford, other factors on Lewis' basis, involving other assumptions as to depth and degree of compensation, can be found which will produce computed deflections more nearly in agreement with the observed deflections

than are those computed by Hayford. This is essentially Mr. Lewis' reasoning.

The tacit assumption is, however, not even approximately true, as shown in the following paragraphs, and hence all reasoning based on the assumption leads to erroneous conclusions.

The reduction factor under discussion is the factor by which topographic deflections for a given ring must be multiplied to secure the resultant deflection due to both the topography and the assumed compensating deficiency or excess of mass below the surface of the earth.

On pp. 26–33 of *The Figure of the Earth and Isostasy* five complete examples of the computations of the topographic deflections are printed. Note that this is one of the publications criticized by Mr. Lewis. His tacit assumption has been tested by using these examples, just as he himself should have tested it.

In the following table the details of the test are shown for two stations. The value shown in the table for each ring is the resultant deflection due both to topography and to the assumed isostatic compensation. The values in the first column which is under the heading "Point Arena" correspond, as indicated in the heading, to the assumption that isostatic compensation is but one-half complete and that it extends to the depth 19.29 km. Each value was obtained by multiplying the topographic deflection for the particular ring, as shown in the example on p. 32 of *The Figure of the Earth and Isostasy*, by the Lewis factor shown on p. 617 of his article. The next column shows the deflections for each ring corresponding to the assumption that the isostatic compensation is nine-tenths complete and the depth as before 19.29 km. The Lewis factors necessary for use in computing this column, as well as those mentioned elsewhere in this rejoinder, were computed from the formula given on p. 615 of the Lewis article. The third column shows the deflections corresponding to the assumption that the compensation is complete and the depth 113.7 km.

According to Mr. Lewis' reasoning the total for the first column under Point Arena, +53″.73, should differ but little from the total for the third column, +15″.67. The actual difference, 38″.06, is very large. Similarly the corresponding difference for Uncom-

pahgre, namely, between $-3''.93$ and $+5''.55$ is also large, $9''.48$. Evidently the factors given as examples by Mr. Lewis on p. 617 did not produce the results expected by him.

Ring	Longitude Station No 1 Point Arena, Cal.			Latitude Station No. 54 Uncompahgre, Colo.		
	Lewis $M=.5$ $h_1=19.29$	Lewis $M=.9$ $h_1=19.29$	Hayford $M=1.0$ $h_1=113.7$	Lewis $M=.5$ $h_1=19.29$	Lewis $M=.9$ $h_1=19.29$	Hayford $M=1.0$ $h_1=113.7$
30	+ ″.02	+ ″.02	+ ″.02	—	—	—
29	+ .04	+ .04	+ .04	—	—	—
28	+ .04	+ .04	+ .04	—	—	—
27	+ .05	+ .05	+ .05	—	—	—
26	+ .02	+ .02	+ .02	+ ″.27	+″.26	+ ″.27
25	+ .02	+ .02	+ .02	+ .28	+.28	+ .29
24	+ .01	+ .01	+ .01	+ .21	+.21	+ .22
23	+ .10	+ .09	+ .10	+ .14	+.14	+ .15
22	+ .12	+ .12	+ .13	− .07	−.06	− .07
21	+ .15	+ .14	+ .16	+ .03	+.03	+ .03
20	+ .14	+ .13	+ .15	+ .05	+.05	+ .06
19	+ .22	+ .19	+ .25	− .33	−.28	− .36
18	+ .25	+ .20	+ .29	− .36	−.29	− .41
17	+ .28	+ .21	+ .34	+ .22	+.16	+ .26
16	+ .43	+ .27	+ .55	+ .67	+.42	+ .85
15	+ .90	+ .45	+1.17	+ .92	+.46	+1.19
14	+1.34	+ .53	+1.69	+1.05	+.42	+1.33
13	+2.11	+ .66	+2.42	+1.27	+.40	+1.45
12	+2.59	+ .68	+2.46	+ .75	+.20	+ .71
11	+3.01	+ .70	+2.11	+ .07	+.01	+ .04
10	+2.90	+ .63	+1.34	− .50	−.11	− .23
9	+3.01	+ .62	+ .83	− .54	−.11	− .15
8	+4.59	+ .95	+ .71	− .02	.00	.00
7	+4.72	+ .95	+ .38	− .42	−.08	− .03
6	+5.05	+1.02	+ .20	− .30	−.06	− .01
5	+4.66	+ .93	+ .09	− .58	−.12	− .01
4	+5.04	+1.01	+ .05	− .82	−.16	− .01
3	+5.02	+1.00	+ .03	−1.34	−.27	− .01
2	+4.09	+ .82	+ .01	−2.04	−.41	.00
1	+2.81	+ .56	+ .01	−2.54	−.51	− .01
Totals	+53.73	+13.06	+15.67*	−3.93	+.58	+5.55*

*The slight discrepancies, too small to be of any consequence, between these totals and those printed on p. 55 of *The Figure of the Earth and Isostasy* are due to the effects of omitted decimal places.

Possibly it might be said by Mr. Lewis that this failure to secure a close agreement is due to having made so great a reduction in the assumed completeness of compensation as to much more than counteract the reduction in the assumed depth of compensation. Accordingly the middle column has been inserted in the above

table corresponding to the assumption that the compensation is nine-tenths complete and the depth is 19.29 km. This gives a closer agreement in each case, as should be expected, since the assumption is nearer to that made by Hayford, but still the differences from Hayford's values are large—2″61 for Point Arena and 4″97 for Uncompahgre.

If Mr. Lewis had followed out his own line of thought, when he reached this point he should have inquired what value of M, expressing in his notation the completeness of compensation, would make his values agree with Hayford's values. If he had done so he would have found that a close agreement would occur for Point Arena with M assumed to be about .87 and thus the truth of his tacit assumption would apparently have been confirmed.

Again, if he had tried Mt. Ouray he would have found that with $M=.2$ and $h_1=19.29$ km. the total computed deflection is $-10''02$, practically in agreement with the Hayford value. Note the contrast between the reduction to compensation only one-fifth complete necessary at this station to neutralize the effect of the assumed reduction in depth of compensation and the comparatively slight reduction, to .87, necessary at Point Arena.

But if Mr. Lewis had continued still farther and tried the same thing for Uncompahgre he would have found that for $M=1.34$ and $h_1=19.29$ km. the total computed deflection is $+5''47$, practically in agreement with Hayford's value. The value 1.34 for M indicates 34 per cent of over-compensation. In other words, for Uncompahgre it takes an assumption of one-third over-compensation to neutralize the assumption of a reduction of depth of compensation, whereas Mr. Lewis' whole argument is based upon the assumption that under-compensation is always necessary for such neutralization.

The following table shows the results of the tests made in preparing this rejoinder using the five stations[1] which were available to Mr. Lewis. Each of the Lewis values which lies nearest to the Hayford value for that station is printed in italics. The values

[1] For all the 733 stations involved in the investigation the total topographic deflections are printed, but those for each separate ring are printed for these five stations only. Hence these are the only stations on which it was possible for Mr. Lewis, using publications only, to make the test indicated.

in the table are the computed deflections on the different assumptions indicated. They correspond to the totals at the foot of the preceding table.

	Longitude Sta. No. 1, Point Arena, Cal.	Azimuth Sta. No. 59, Mt. Ouray, Colo.	Latitude Sta. No. 54, Uncompahgre, Colo.	Latitude Sta. No. 164 Calais, Me.	Azimuth Sta. No. 115, Knott Island, Va.
Hayford					
$M=1.0$, $h_1=113.7$	+15″.67	−10″.03	+5″.55	−0″.51	− 1″.90
Lewis					
$M=0.2$, $h_1=19.29$ ...	......	*−10.02*	....	....	....
$M=0.5$ $h_1=19.29$ ...	+53.73	− 8.22	−3.93	−16.00	−27.19
$M=0.9$ $h_1=19.29$ ...	*+13.06*	− 5.86	+0.58	*− 2.97*	*− 5.52*
$M=1.34$ $h_1=19.29$ ...	......	......	*+5.47*	......	......
$M=1.4$ $h_1=19.29$ ...	......	......	+6.19	......	......
$M=1.6$ $h_1=19.29$ ...	......	......	+8.36	......	......

If Mr. Lewis had constructed this table, which would have required less than a day of computation, he would have avoided the gross error into which he has fallen. Instead of believing, as indicated in his article, that a large reduction in assumed completeness of compensation of about the same amount for every station would neutralize the large change of assumed depth from 113.7 to 19.29 km. he would have known that for some stations such a neutralization requires a very large reduction of assumed completeness (to $M=.2$ at Mt. Ouray, for example), for others requires a very small reduction (at Point Arena, Calais, and Knott Island, for example), and for others requires an increase of assumed completeness, that is, over-compensation (at Uncompahgre, for example). He would have noticed that for these five stations the average value of M necessary to secure an agreement with the Hayford values is about .9, not far from unity.

If beginning to be skeptical of his own proposition that a reduction in completeness of compensation at all stations would counteract a reduction in assumed depth of compensation, and beginning to suspect that such a counteraction is impossible in the computations as actually made, he had then proceeded to examine into the matter more carefully he might have noted also the following things:

1. That the factors as printed on p. 617 differ very largely for separate rings from the Hayford factors. The maximum

difference is .499, whereas the factor itself can have a total range of only 1.000.

2. That for other various assumed values of M, with the same value of h_1, his factors always differ largely from Hayford's for some rings. If $M = .9$, for example, the maximum difference occurs on ring 14 and is .496.

3. That to secure a close agreement in the computed deflections from two sets of reduction factors used at various stations there must be a close agreement of the factors for the separate rings, not simply an agreement in the average values of the two sets of factors, since the computed deflections for the various rings at a given station differ greatly.[1] At Point Arena, for example, the topographic deflections for various rings vary from $+\rlap{.}''01$ for ring 24 to $10\rlap{.}''10$ for ring 6, and at Uncompahgre from $+2\rlap{.}''35$ for ring 13 to $-5\rlap{.}''07$ for ring 1. These topographic deflections for separate rings are the quantities which are multiplied by the reduction factors to obtain the computed deflections. Evidently at Uncompahgre a decrease in the reduction factor in ring 13 will not be partly neutralized in effect by an increase of the factor for ring 1, but instead the two effects will be of the same algebraic sign, since the topographic deflections for these rings happen to be of opposite signs.

4. That the proposed Lewis factors corresponding to incomplete compensation are always greater than the Hayford factors for the extreme outer rings (distant topography), and less than the Hayford factors in the inner rings corresponding to topography at a moderate distance. This gives a clue to the reason for the fact that at some stations it requires a decrease in assumed completeness and at other stations an increase to counteract an assumed decrease in depth of compensation. In this connection it is important to note that the 733 stations used in the computation being criticized are in a great variety of locations with reference to near and distant topography.

5. That following the clue suggested in (4), it becomes evident that there is no fixed relation between the effect upon the compu-

[1] Consult pp. 26–33 of *The Figure of the Earth and Isostasy.*

tations of a reduction in an assumed completeness and a reduction in depth.

6. That in a computation such as that being criticized, based upon 733 stations all utilized fully in a single computation, though an error of moderate size in assumed completeness of compensation will produce residuals at the separate stations, it will have an exceedingly small, probably inappreciable, effect on the computed depth of compensation.

7. That the residuals from the computation actually made, which residuals are printed in detail, show that the assumptions are in very close agreement with the truth, and especially that it is impossible that there is any large error in the assumed completeness of compensation.

8. Finally by following the clues indicated in (4) it would be noted that if it were a fact that the actual depth of compensation is much less than 113.7 km. and the compensation much less than complete the residuals of the accepted final computation would show a certain systematic geographic distribution. For example, all longitude stations and all azimuth stations situated on or very near the Pacific coast, like Point Arena, should have residuals of the same sign as Point Arena. The geographic distribution of residuals which would be so produced does not exist.[1]

In short, if Mr. Lewis had carefully examined the evidence available to him, following logically the lines of thought on which he started, he would have reached the conclusion that an error of moderate size in assumed completeness of compensation would produce no appreciable error in the depth of compensation as actually computed, and that the actual compensation certainly departs but little from completeness.

It should be clear from what is here printed that the alleged error in Hayford's method of computation on which Mr. Lewis' whole criticism rests is fictitious. Even if it is not clear to one who reads this very brief statement, it will certainly become perfectly clear to those who will examine the methods in detail with the numerical values before them.

[1] Consult illustrations 5 and 6 of the *Supplementary Investigation of the Figure of the Earth and Isostasy.*

Mr. Lewis suggests more than once in his article that possibly the existing condition is "an over-compensation at a greater depth for land areas with probably complete compensation for ocean areas" or "under-compensation at a shallower depth for land areas with complete or over-compensation for ocean areas" (p. 626).

If Mr. Lewis or anyone else will carefully test these ideas in the manner indicated on pp. 563–70 of this rejoinder he will certainly reach the conclusion that there is nothing in the suggestion. The writer had made such tests, and others, before Mr. Lewis' article was written.

To test such suggestions by complete computations, such as those set forth in Hayford's publications which are under criticism, would be a waste of time. It is not worth while to spend months in testing a suggestion by a complete computation if, as in this case, a rough test made in one or two days will show the suggestion to be in error.

Turn now to another direct and positive statement made by Mr. Lewis, which is closely allied with the questions discussed above. After stating, correctly, that all Hayford's principal computations were made on the assumption that the isostatic compensation is complete, Mr. Lewis continues thus (pp. 610–11), "This was a purely arbitrary assumption on Hayford's part since he gave no reason whatever for believing at the outset that compensation is complete, and furthermore the fact that he later attempts to find the degree of completeness implies that there is no reason to believe at the outset in complete compensation."

Hayford has indicated in various places in his publications that gravitation tends to produce a readjustment of the material composing the outer portion of the earth toward the condition of approximate equilibrium known as isostasy.[1]

Gravitation acts continuously. It certainly tends to produce complete compensation. Is it purely arbitrary to assume complete compensation as a first approximation? The writer believes it is not.

[1] Consult especially *The Figure of the Earth and Isostasy*, pp. 66–67, 166–68, and the Minneapolis address referred to in the first paragraph of this rejoinder. In this address the possible mechanics of the readjustment is indicated.

Moreover, it requires but little consideration to realize that the known shiftings of load at the surface tends to bring about over-compensation in some localities and under-compensation in others. For example, long continued erosion from a high mountainous region tends to produce over-compensation and deposition by a river of transported material at points above sea-level, as for example, during the raising of the land surface of a delta, tends to produce under-compensation. Various actions below the surface of the earth known to geologists and others tend to produce over-compensation in some locations and under-compensation in others. Gravitation by its continuous action tends, therefore, to cause the condition to approach complete compensation from the side of over-compensation in some localities and from the side of under-compensation in others. Gravitation is resisted by rigidity and it is, therefore, to be expected that it will be but partially successful in the attempt to produce complete compensation. The writer believes, therefore, that it was logical to assume that the compensation is complete rather than that either under-compensation or over-compensation predominates. The assumption was made, not arbitrarily, but logically.

However, it is clear that as long as the material composing the earth has some rigidity, some strength available to resist gravitation, and as long as other forces than gravitation certainly are in operation, complete compensation cannot be continuously produced and maintained by gravitation and one must expect local deviations, now of one sign now of the other, from complete compensation. Hence it was desirable to study the residuals from the computations made on the basis of complete compensation to ascertain if possible how much and in what direction the actual compensation departs from completeness in each locality. This has been done with considerable energy. From the investigations of the deflections the conclusion reached is that there is certainly under-compensation in some localities and over-compensation in others but that on an average the compensation departs less than one-tenth from completeness or perfection.[1] A similar conclusion

[1] *The Figure of the Earth and Isostasy*, pp. 164–66, 175, and *Supplementary Investigation*, p. 59.

is reached from the gravity investigation except that the departure of compensation from completeness is somewhat greater than one-tenth.[1]

Turning now to Mr. Lewis' article the present writer is quite willing to let the reader decide whether or not Mr. Lewis' assumptions (p. 626) of over-compensation for all land areas, or of under-compensation for all land areas and over-compensation for all ocean areas, are arbitrary.

The preceding parts of this rejoinder have dealt with those portions of Mr. Lewis' article, which are a discussion of the geodetic evidence of isostasy. Turn now to the portions of his article in which certain geologic evidence is interpreted as being adverse to the existence of isostasy.

On pp. 621–22 Mr. Lewis sets forth the argument that there is much geological evidence of horizontal movements in the outside portions of the earth especially in the form of folding, that the controlling movements of isostasy are assumed to be vertical and hence cannot account for folding, and that the horizontal movement or undertow concerned in isostatic readjustment must be below the depth of compensation and hence so far below the surface as to be very ineffective in producing folding.

There are two fatal defects in this argument as applied to controverting anything that Hayford believes or has written.

First, Hayford has already indicated clearly his belief that the undertow concerned in isostatic readjustment is above, not below, the depth of compensation. In both the figures published in his Minneapolis address the undertow is clearly indicated as being above the depth of compensation and it is also so indicated in the corresponding text. As Hayford puts the undertow comparatively near the surface where it is conceded that it would be effective in producing folding, the existence of extensive folding is a confirmation not a contradiction of his theory of the manner in which isostatic readjustment takes place. It is certainly not fair to hold Hayford responsible, either directly or by inference, for any theory which someone else may believe which involves an undertow

[1] See p. 111 of *Special Publication No. 10*, of the Coast and Geodetic Survey.

situated entirely below the depth of compensation. Mr. Lewis apparently believes such a theory.

Second, the movements which produce isostatic readjustment are necessarily horizontal not vertical. If two adjacent columns of the same horizontal cross-section extending from the surface to the depth of compensation have different masses the readjustment to perfect compensation must involve a transfer of mass out of one column, or into the other, or from one to the other. In any case the transfer must be a horizontal movement, though it may be incidentally accompanied by a vertical movement. Hayford has already shown in print more than once that he understands that vertical movement alone does not produce isostatic readjustment. Moreover, a careful reading of his Minneapolis address will certainly show that he believes that the total amount of material moved horizontally during isostatic readjustment, and especially the total number of ton-miles of such movement, is vastly in excess of the corresponding quantities concerned in the vertical components of the movement which takes place. Hence the folding and other abundant evidence of past horizontal movements observed by geologists confirm Hayford's hypothesis as to the manner in which isostatic readjustment takes place, instead of conflicting with it as Mr. Lewis' article would lead one to think.

In contrast to the paragraph at the top of p. 623 of Mr. Lewis' article in which he claims that "the theory of isostasy does not explain the apparently heterogeneous relation of uplift and subsidence to erosion and deposition," the writer respectfully requests that certain parts of his Minneapolis address be considered in which it is set forth at some length that the movements concerned in isostatic readjustment at a given time and place are probably a function not simply of the facts at that time and place but also of the past facts there and of the current facts at many other places, some at a considerable distance, perhaps hundreds of miles away. If this be true, one should not expect to find a fixed relation of uplift and subsidence at any given point or time to the erosion or deposition in progress at that point at that time. Why does Mr. Lewis ignore this contrast?

So too, when one reads Mr. Lewis' statement (p. 623) that "in

some cases erosion to a peneplain has been followed by subsidence and in other cases by uplift" and notes that the context seems to imply that this cannot be reconciled with the theory of isostasy, one is puzzled to understand why no reference is made to that part of Hayford's address in which two long paragraphs are devoted to setting forth the errors in this particular line of reasoning.

Thus far in this rejoinder only such evidence has been cited as was available to Mr. Lewis when he wrote his article. In May, 1912, the Coast and Geodetic Survey issued a publication entitled *The Effect of Topography and Isostatic Compensation upon the Intensity of Gravity*, written by John F. Hayford and William Bowie. This contains further evidence pertinent to the questions under discussion.

In the principal computations set forth in this new publication the effects of isostatic compensation upon the intensity of gravity are computed upon the assumption that the compensation is complete and is uniformly distributed to the depth 113.7 km. The depth was adopted from the first figure of the earth investigation by Hayford and the assumption of complete compensation was retained.

Let the evidence furnished by this new publication on gravity now be considered in connection with Mr. Lewis' article.

In the new gravity publication the computations are made by concentric circular zones as in the computations of topographic deflections, but the zones are not the same. In the new publication in the table on p. 100 the effect upon gravity, at each of 41 stations in the United States, of the assumed compensation for all zones out to zone O, covering all areas within 166.7 km. of the station, is tabulated separately from the effect of the topography itself. This enables one to apply two tests as indicated below.

First, let it be assumed that for these 41 stations the compensation is only nine-tenths complete. Then the computed effect of the compensation as shown in the table should in each case be diminished by one-tenth of itself. This would produce a contrary change of the same magnitude in the anomaly, or residual, shown in the fourth column from the last in this table. An inspection of the values shows that for the 41 stations 19 residuals would be

reduced by such a change, 20 increased and two left as before. The test thus gives a neutral result.

A similar test was made on the assumption that the compensation is only one-half complete, as suggested by Mr. Lewis. Such a change would increase 29 of the 41 residuals. This is a strong indication that incompleteness of compensation does not exist to that extent.

But Mr. Lewis might say in this connection, as he did in connection with the deflections of the vertical, that an error in assuming the compensation to be complete had been offset by an error in making the depth of compensation too great. On p. 105 of the new publication there is a table showing the effect upon the computed correction for compensation of changing the assumed depth of compensation from 113.7 km. to 85.3. The ten stations in this table are also in the table on p. 100. Hence it was possible by a comparison of the two tables to compute the value which must be assigned to Mr. Lewis' quantity *M*, expressing the completeness of compensation, to make the assumed change in completeness at each station counteract the assumed change in depth. The comparison was made and it was found that the ten necessary values of *M* vary for these ten stations from .56 to 1.78, with a mean of about unity. This showed, just as it has already been shown earlier in this rejoinder, in connection with deflections of the vertical, that there is no fixed relation between the effects due to changes of assumed depth of compensation and those due to changes of assumed completeness.

Both these tests are incomplete because they utilize only a few of the 89 gravity stations and because they utilize only that portion of the compensation which is within 166.7 km. of the station. But the tests give sufficiently decisive results to show that further investigation along this line is reasonably certain to be fruitless.

The most important confirmation by the gravity publication of conclusions previously drawn from deflections of the vertical, is that shown on pp. 117–21 of the gravity publication. From studies of deflections of the vertical 11 specified areas of excessive density (under-compensation) had been located in the United

States and five specified areas of deficiency (over-compensation). The gravity observations confirm the existence of 10 of these 16 anomalous areas, there being a confirmation in every case in which the gravity stations were so located as to give a thorough test. In no case was there a contradiction between the conclusions from the gravity observations and those from observations of deflections of the vertical. If the apparent residuals in each case were due to errors in assumptions, as contended by Mr. Lewis, this confirmation could not occur. In that case the geographic distribution of the residuals would certainly be different in the two cases for the reasons indicated in the following paragraph.

The intensity of gravity at a given station is a summation of the vertical components of the gravitational forces at that point minus the centrifugal force due to the earth's rotation. The deflection of the vertical at a station dealt with in the publications criticized by Mr. Lewis depend, on the other hand, upon the summation of the horizontal components of the gravitational forces at a given point. The pendulum responds mainly to masses which are above or below it. The plumb bob responds mainly to masses which are to the right or left, before or behind the station. Hence any errors of assumption as to the distribution of the isostatic compensation necessarily produce different effects in connection with gravity computations than those same errors of assumption produce in connection with computations of the deflections of the vertical. Hence a study of the two kinds of computations for the same region furnished a very severe test for errors of assumption. It is especially important to note that if a given error of assumption produces residuals in an investigation of deflections of the vertical having a certain geographic distribution that same error of assumption would produce a different distribution of residuals in an investigation of gravity.

In the new gravity publication the computed corrections for the effects of topography and compensation are published in such detail, for every zone at every station, that Mr. Lewis or anyone else has abundant opportunity to test the effects of making the assumptions different from those upon which the computation

was based. The authors of the gravity publication feel confident that such tests, if thoroughly made, will show that the assumptions in the published computations are very near the truth.

The fact that the gravity observations confirm all the conclusions previously drawn from deflections of the vertical is an exceedingly strong indication that there are no fundamental errors in the method of computation used in either investigation.